sufficient, still the method is only available on a comparatively clear night; and though the same sights may possibly be also used for the determination of longitude, it will more commonly happen that the complete position may be satisfactorily determined by Sumner's method applied to two stars having a considerable difference in azimuth.

The pages in which Mr. Rosser treats of Sumner's method are of themselves sufficient to establish what has been already said as to the practical nature of the book. In an admirable monograph published two years ago, under the title of "Stellar Navigation," Mr. Rosser has shown himself alive to the very great value of this method of determining a ship's position, and to the necessity of shortening the calculation by the use of Sir William Thomson's special tables, or by Burdwood's and Davis's azimuth tables. But no remark in the "Self-Instructor" calls attention to this, and the problem is left, in its native clumsiness, in the form suitable to the questions of the examination room. The same might indeed be said of almost all other problems, which are given without any hint of the little artifices which, in practice on ship-board, render the computation quicker and easier. In saying this, however, we attach no blame to Mr. Rosser, unless it is for calling his book "practical," or "adapted for use at sea." The book is meant to meet the demands of the examinations; and for this, at least, it appears sufficiently well adapted.

J. K. L.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

On the Cause of the Dissimilarity between the Faunas of the Mediterranean and Red Seas

THE republication by Mr. A. H. Cooke of the list of Testaceous Mollusca obtained by the late Mr. Robert MacAndrew during a dredging excursion (in 1869) in the Gulf of Suez,[1] affords data for comparison with that of the Mediterranean over its eastern part, and of which the late Mr. J. Gwyn Jeffreys has, amongst other writers, given an account.[2] The extreme dissimilarity in reference to the species will, upon such a comparison, impress the mind.[3] I propose briefly to sketch out the process by which this dissimilarity may be supposed to have been brought about.

Going back to the Eocene period, we know that the whole of the region bordering the Levant, and including large portions of the three continents, formed the bed of the ocean, and we may presume that a community of genera and species existed over the whole tract represented by those of the Nummulite limestone of the Middle Eocene period.

During the Upper Eocene period there was a shallowing of the sea-bed in many places, and corresponding deepening in others, and thus the first division of the submerged area into deep and shallow basins would have been brought about with a certain influence on the animal and plant life; but the general result may not have been considerable.

It was during the succeeding Miocene period that the differentiation of the fauna and flora of the two seas really began. Recent observations on the geology of Northern Africa, Arabia, and Palestine by Zittel, Lartet, and others, leave little doubt that the Miocene period was one during which the main lines of the future lands and seas were marked out; and the absence of deposits belonging to this epoch (except a few scattered tracts formed of shallow-water and littoral beds) over the region referred to, leads to the conclusion that land-conditions prevailed very much where we now find them, and that the submerged areas of the Mediterranean and Red Seas were dissevered by the Isthmus of Suez. It was during this period of elevation that the differentiation proceeded; the original forms of the Eocene period developing in each basin independently of one another, and becoming more divergent as time went on. The process seems to have been continued well into the Pliocene epoch, but at a time which may be indicated perhaps as "Newer Pliocene" there occurred a re-submergence of the land to the extent of 220 to 250 feet below the present level of the sea, marked by the occurrence of raised sea-beds containing shells, &c., of species still living in the adjoining waters, and of old coast-cliffs perforated by Pholas borings, like that discovered by Oscar Fraas in the cliffs of Jebel Mokattam, near Cairo, at an elevation of 220 feet above the surface of the Mediterranean, and recently described by Dr. Schweinfurth (*Zeitsch. d. deutschen geolog. Gesellschaft*, 1883). During this depression Africa became an island, and the waters of the two seas were united.

With this union of the Mediterranean and Red Seas there must have been brought about a certain commingling of the forms inhabiting their waters respectively, and hence it is somewhat surprising that there should at the present day be found such an almost entire dissimilarity as that already stated. The explanation, it seems to me, is to be found in the fact that the strait was, in its shallower portion, very shallow; and that consequently, except for the purely littoral and shallow forms of marine life, a commingling really did not take place to any great extent. To the north of Lake Timsah there occurs a ridge of ground called *El Guisr*, which rises 70 feet above the present sea-level, and another called *Tunum*, which rises 25 feet. These ridges would have caused a shallowing of the strait to the extent of their elevation, so that over the former ridge the depth of the strait would only have amounted to 180 feet or less during the greatest submergence. It is impossible to say whether these ridges are higher, or the contrary, than they were at that period; but it is a remarkable fact that the sub-fossil shells in the gravels to the south of Tunum are those of the Red Sea, and to the north those of the Mediterranean; other ridges, like that of Tel-el-Kebir, produced similar shallows. As a general result it is clear that the submersion of the isthmus during the later Pliocene period did not produce a general commingling of the forms of the two seas; and when ultimately the seas were again separated by the re-elevation of their beds, and the present isthmus established, those forms which may have passed across from sea to sea would succumb to the altered conditions of their environment. It can scarcely be doubted that the temperature of the water of the Red Sea differs considerably from that of the Mediterranean by several degrees, and the forms which belong to the former would perish in the latter, and *vice versâ*. It would be interesting to ascertain which of the two faunas more closely resembles that of the original Eocene stock.

Here, then, we have the remarkable zoological phenomenon of two perfectly distinct sets of marine forms originating in one stock only as far back as the Middle Eocene period, independently developing to such an extent that, at the present day, there are scarcely more than eighteen species (according to Prof. Issel) common to both. Now, if the beds of these two seas (the Levant and Red Sea) were to be elevated into land and their fossil contents studied by a geologist of the future, he would probably assert on the palæontological evidence that they belonged to two distinct periods of geological time! This is subject matter for reflection, at least for geologists of the present day. I may add that I have been induced to try and solve to my own satisfaction the problem here presented while engaged on a work containing the scientific observations and conclusions made during the recent expedition to Arabia Petrea in connection with the "Palestine Exploration Fund."

EDWARD HULL

[1] *Annals and Magazine of Natural History*, vol. xv. p. 322 (fifth series).
[2] *Ibid.*, vol. vi. p. 65 (fourth series).
[3] This fact has been recognised by Prof. Haeckel in his "Visit to Ceylon" and his "Arabische Korallen," &c.

Hybridization among Salmonidæ

I PERCEIVE in NATURE (vol. xxxi. p. 563) that the "National Fish Culture Association" propose cross-breeding land-locked salmon and trout as proposed by Prof. Brown Goode in "Forest and Stream," August 7, 1884. Before doing so I would venture to direct their attention to a few points.

"Land-locked salmon" is admitted to be a race of the true *Salmo salar*, which from some cause having lost its migratory instinct, now lives in lakes, never migrating seawards, while its size is less than that of its sea-going relative. But as the two species are really the same, a cross between a land-locked salmon and a trout in fish-cultivation would be identical with a cross between a *Salmo salar* and a trout.

What then has been the result of attempting the latter cross at Howietown during the last few years? November 25, 1879, this was effected between salmon milt and Lochleven trout eggs; up to now all the offspring have been sterile, none have attempted to spring out of the ponds, and the largest fish among them last year, although in good condition, was only 16½ inches long. On December 24, 1881, this cross was again made, with similar results, the largest fish last winter being about 12 inches long. (Examples are in the South Kensington Museum). Sterility, I may remark, was anticipated from this cross, while it was supposed that such would remove the anadromous instinct, and these results have occurred, but as regards improvement in size, such has not, so far, proved a success.

A cross was made between a young salmon par and a Lochleven trout, on November 29, 1883, but the young succumbed to blue dropsy of the sac. This cross was again tried November 14, 1884, when the par was a year older, and so far the young look well, but we can scarcely anticipate their proving fertile offspring. I say "scarcely," for we know that domestication eliminates sterility in some races of hybrids, and in this instance the par had been raised from eggs at Howietown; these have now grown into grilse without descending to the sea, and given eggs. Eggs thus furnished from Howietown-raised grilse have hatched, and several thousand young par are in the establishment, the future of which race will be an interesting study.

I think I am justified in advising that when crossing salmon with trout, not to select a parent from a river or lake, but, if possible, to obtain eggs or milt from a race of salmon which has been two or more generations in a semi-domesticated condition, as with such the probabilities of failure are considerably lessened, but, so far as I have witnessed, hybrids between salmon and trout have proved sterile and undersized.

Cheltenham FRANCIS DAY

Forms of Leaves

IN a recent issue of NATURE, in the discussion on the forms of leaves, Mr. Henslow seems to doubt the assertion of Sir John Lubbock that the holly produces prickly leaves on the lower branches, and smooth leaves without spines above; but this is a fact which may easily be verified in numerous localities (selected gardens varieties are of course not intended). I know of a large tree at Kew which altogether confirms the statement. The explanation, however, that the spines of the lower leaves may be produced to prevent animals from browsing on them, and that they are not developed on the upper branches because these are beyond the reach of animals, seems to me to require some modification, if not to be given up altogether, in this limited sense. It seems to me to admit of a much simpler explanation, namely, that it is an approximation—or reversion, if indeed the term be applicable—to the ancestral type. It is a well-known fact that in the embryonic stage of an organism the affinity with the ancestral type is best seen, and that in the mature stage the greatest amount of specialisation takes place; and, viewed in this light, the case of the holly does not appear to present much difficulty. A young seedling is seen to have very spiny leaves, but with increasing age the leaves becoming comparatively spineless. In the case of the furze we have the most overwhelming evidence that the spiny character has been developed to repel the attacks of herbivorous animals, and a young seedling is seen to have trifoliate leaves—like the laburnam—from which we infer that its ancestral type was spineless, and had trifoliate leaves. The large group of phyllodineous Acacias bear an equally unmistakable stamp of their origin in the bipinnate leaves which the seedlings at first produce. In most cases these leaves are very early superseded by phyllodes, but in *A. melanoxylon* the habit of producing true leaves is never quite lost. There is a large tree of this species about 40 feet high at Kew, at the south end of the Temperate House, close to the spiral staircase. It is thus in an admirable position for examination. At the base of this tree the leaves predominate over the phyllodes, but in ascending the staircase the proportion is seen to gradually diminish, till at the top of the tree—a few feet above the gallery—scarcely a true leaf is to be seen. Assuming the mature stage to be the more highly specialised, we have in the holly a precisely parallel case. This necessarily involves the opinion that the ancestral type of the genus *Ilex* had spiny leaves; and, if so, it seems highly probable that the character was developed as a protection against the attacks of herbivorous animals. A possible objection which at first struck me was that many of the species have quite smooth leaves; but this has been removed by a search through the specimens in the Kew Herbarium. In the first place, species with spiny leaves occur in each great centre of distribution of the genus—in North and South America, India, China and Japan, the Atlantic Islands, as well as Europe—and in the second, although no seedling plants were found, there are three species which show very spiny leaves on barren branches, and smooth leaves on the more mature flowering branches. These are *I. insignis* and *I. dipyrena*, from India, and *I. Perado*, from the Atlantic Islands. I have little doubt that seedlings of many species would present the spiny character if we could only see them. The presence of spines—the nerves being extended beyond the margin of the leaf—seems to indicate an excess of vascular over cellular tissue; a condition which is either modified with increasing maturity or is not exhibited in the same phenomena. In any case a severe pruning—or reduction of the parts to be nourished—is followed by a temporary reversion to the more spiny character. If this explanation be the correct one the question naturally arises, Why are the hollies losing the property of producing spiny leaves? rather than, Why does the holly produce spiny leaves on its lower branches? The answer to the first query would perhaps be, Because they no longer need the protection afforded by the spines. To the second, Long-continued habits are not often instantly laid aside.

Herbarium, Kew, April 18 R. A. ROLFE

Kite-Wire Suspended Anemometer Readings

HAVING lately made some observations with my anemometers elevated, as above described, at heights above the ground considerably greater than those mentioned in my paper before the British Association last year, I venture to think that a word or two as to the main point at present under investigation, viz. the general increase in the velocity with the altitude at heights between 600 and 1100 feet above the ground, may be interesting to your readers.

Up to June last the greatest altitude reached by the anemometers was 646 feet. I have lately been able to secure readings up to 1129 feet. Taking the average of seven of these, we get the following values for the mean relative velocities at two mean heights:—

Height in feet above ground.	Velocities in feet per minute.
1070	2297
756	2165

When these values are inserted in the formula $\frac{V}{v} = \left(\frac{H}{h}\right)^x$, we get for the value of the exponent $x = 0{\cdot}17$, or a little more than $\frac{1}{6}$; but when 500 feet—the elevation of the place of observation above the sea—are added to each elevation, we get $x = 0{\cdot}26$, or almost exactly $\frac{1}{4}$, which is the value I deduced for the exponent in NATURE (vol. xxv. p. 506), from a discussion of Dr. Vettin's cloud observations.

I would not at present lay much stress upon this coincidence until I have investigated the ratio up to heights of 2000 feet or more, but I certainly think it supports the notion that the formula with this exponent represents the average law of increase at heights over 1000 feet above sea-level.

E. DOUGLAS ARCHIBALD

Temperature of the Body of Monotremata

I HAVE found the temperature of the body of *Echidna hystrix* to be (average of three observations) 28°·0 C., and that of *Ornithorhynchus paradoxus* (two observations) 24°·8 C.[1]

These temperatures present a special interest, comparing them with the mean temperature of the body of mammalia in general, which is (after Dr. J. Davy's observations of thirty-one different species) 38°·4 C. N. DE MIKLOUHO-MACLAY

Biological Station, Watson's Bay, near Sydney, N.S.W., March 10

[1] Details of these observations can be found in the *Proceedings* of the Linnean Society of New South Wales, vol. ix. pp. 425 and 1204.